TABLE 1*

Frogs	Sex	Av wt of frogs	Av wt of thyroids	Av wt of livers	Av wt of liver slices	Av wt of gastrocnemius muscles	Av wt of muscle slices	Av count of thyroids	Av count of liver slices	Av count of muscle slices
8	♂	20.3	0.00036	0.91	0.00183	0.72	0.00200	1712	808	475
4	♀	19.9	0.00034	0.93	0.00201	0.67	0.00216	1720	832	431

* The mean values in grams for the weight of the frogs, of the thyroid glands, livers, liver slices, gastrocnemius muscles, and muscle slices of separate groups of male and female frogs; also the mean values for radioactivity counts/2-min interval for the thyroids, liver slices, and muscle slices.

screened cage under dripping water in an air-conditioned room at 5° C. Groups of animals were given intraperitoneal injections of approximately 22.5 μc of carrier-free radioactive iodine in a sodium bisulfite solution. The animals were sacrificed 4 hr after the injection of the radioactive iodine. The thyroids were removed, rinsed in frog Ringer's solution, blotted, weighed, and pressed flat on clean microscope slides; radioactivity counts were made for 2 min with a Geiger-Müller counter. Next, the liver and gastrocnemius muscles were removed from each animal, rinsed in frog Ringer's solution, blotted, and weighed. Thin slices of liver and striated muscle tissue were made with a razor blade, weighed, and prepared for reading as were the thyroids.

The results of these experiments are shown in Table 1. On the basis of recovery of radioactivity, Table 1 shows that the radioactive iodine uptake of the thyroid glands is greater than that of the liver slices, and the uptake of the liver slices is greater than that of the slices of striated muscle.

The weight of the thyroid glands averaged 0.00035 g, the liver slices 0.00192 g, and the striated muscle slices 0.00208 g. The average amount of radioactivity given off for the thyroids was 1716 counts/2 min, for the liver slices 820 counts/2 min, and for the striated muscle slices 453 counts/2 min. Although the average weight of the liver slices is 5.48 times that of the thyroids, and the average weight of the striated muscle slices is 5.94 times that of the thyroids, the thyroids gave off 2.09 times as much radioactivity as the liver slices and 3.78 times as much radioactivity as the striated muscle slices. A comparison of the liver and striated muscle slices shows that, on the average, the muscle slices are 1.08 times as heavy as the liver slices, but the average amount of radioactivity given off by the liver slices is 1.81 times the average for the muscle slices.

The findings reported here indicate that there is a differential iodine uptake by thyroid, liver, and striated muscle tissue of the frog. These findings for thyroid and liver tissue are similar to those of Perlman, Chaikoff, and Morton (*1*) for the rat, and of Chaikoff and Taurog (*2*) for surviving slices of thyroid and liver tissue of sheep. Similar differences between the activities of thyroid and other tissues have been observed by Hertz, Roberts, and Evans (*3*) and by Salter (*4*).

The findings of these experiments may be summarized by stating that the iodine-concentration capacity of thyroid, liver, and striated muscle tissue of the frog, as measured by the uptake of radioactive iodine, showed that the frog's thyroid tissue had the greatest avidity for iodine, liver and striated muscle tissue following in that order.

References

1. Perlman, I., Chaikoff, I. L., and Morton, M. E. *J. Biol. Chem.*, **139**, 433 (1941).
2. Chaikoff, I. L., and Taurog, A. *Ann. N. Y. Acad. Sci.*, **50**, 377 (1949).
3. Hertz, S., Roberts, A., and Evans, R. D. *Proc. Soc. Exptl. Biol. Med.*, **38**, 510 (1938).
4. Salter, W. T. *Ann. N. Y. Acad. Sci.*, **50**, 358 (1949).

Manuscript received September 4, 1952.

Comments and Communications

More on Editorial Prerogatives

(*The following communications are published without editorial alteration.*)

Please refer to "Editorial Prerogatives" (Science, **116** [693–695]).

Now that all other cards probably are down, I'm ready to toss in mine too. Coming this late, my thought will have to make its way strictly on merit, in competition with ideas presumably already formed.

When complaints like this author's are milder, my experience has been that the author has a limited command of English, has not been exposed soon enough in his life to competent editorial work, thinks that the method of expression to which he has accustomed himself is the only possible correct method, and resents any change as if the change were translation into a foreign language. To such an author, an extensive vocabulary and a method of expression different from his own actually are a foreign language.

The rumpus that this author has kicked up over picayune matters, however, suggests in both degree and time element that a good course may be to forget him and concentrate on presenting the following ideas:

Every journal and every printing house has its

own standards. Many, if not most of these standards are identical. An author who does not care to welcome the improvement of competent editing under the standards that apply is privileged to go elsewhere, or to publish privately.

Most technical journals already have more manuscripts than they can publish. Publication in a standard journal is a favor to the author. Every person who reads the printed article shows the author equal courtesy.

A complete engineering drawing shows three views. In a correctly edited manuscript, the editor supplies two of the desirable three points of view, his journal's and the readers'.

A competent editor defends the reader from the author and also the author from himself—and once in a blue moon gets thanks for doing so.

HENRY HENDRICKS KETCHAM
Piqua, Ohio

Your issue of December 19, 1952 presents at some length the pros and cons of the situation brought about by the editors of the Quarterly Review of Biology in their treatment of the manuscript of Dr. K. H. L. Key's important review of our knowledge of locust phases. I infer from your editorial note that free and frank discussion of the matter is expected and welcomed.

As one who has worked for some decades with certain aspects of locust research, and also for some years past has had abundant opportunity to know and judge Dr. Key's fairness and ability and his respect for other authors' rights and prerogatives, it is somewhat of a shock to learn the extent to which his manuscript had been altered, *and put into type,* before he had an opportunity to pass upon these many alterations, and in addition many of these changes allowed to appear over his protest.

Having served for nearly two score years as a member of an important publication committee, and as chairman of two such purely scientific committees producing six serial issues, it is exceedingly difficult for me to understand why a manuscript so greatly altered was not returned to the author for his perusal *before* putting it into type. Granted that the Quarterly Review of Biology works on a very tight schedule, and that its editors are unsalaried and do its work on unofficial time, it is in these respects no different from numerous other journals which could be mentioned.

In the past few years I have been having extensive lots of manuscript and proof pass back and forth between Philadelphia and Australia, accompanied by almost weekly relevant air mail letters, these concerning a monograph being prepared here and published by the Commonwealth Scientific and Industrial Research Organization, with Dr. Key the active intermediary and representative of that body. I can say with full knowledge there has been the most courteous and careful regard for the author's wording, and no change, no matter how minor, is made without full air mail consultation *before anything goes into type.* This may be considered in certain directions as a personal and irrelevant matter, but it reflects, at that end, an attitude of mind which I have always maintained as fundamental, i.e. the author's right to the presentation of his arguments as he sees them, subject only to the correction of errors of statement or direct misuse of English. The alternative is the return of the manuscript.

There is in the rejoinder of the editors of the Quarterly Review of Biology at least one statement unworthy of them. Naturally Dr. Key knew exactly what he wished to say and that he felt he was right. It is, or should be, assumed by all publication bodies that this is true of all scientific authors, otherwise they would not, or should not write. Clearly, as the Quarterly Review of Biology editors admit, if 54.4% of the 287 changes made in Dr. Key's manuscript were allowed to stand over his protest made after seeing them for the first—in type—they are obligated to publish one or the other of the two explanatory statements suggested by them, using that which Dr. Key would prefer. This unfortunate and sweeping exercise of editorial power bring more clearly into focus something which has been hanging, like the sword of Damocles, over the biological world, i.e. the regimenting and bureaucratic tendencies which are endeavoring to mold and standardize to a set pattern the scientific publication media in this country. We can only imagine what the reactions of some of our really great scientific master minds of the past, such as Joseph Leidy, E. D. Cope, Elliott Coues and W. M. Wheeler, as well as many others, equally masters of the written word, would have been to such unrestrained use of the blue pencil, with its consequent alteration of their thoughts and intent.

JAMES A. G. REHN
The Academy of Natural Sciences of Philadelphia

The editorial controversy between Dr. Key and the Quarterly Review of Biology is regrettable, but it may be a real service to the technical press to have aired it. Dr. Key's basic complaint, that of over-editing, is one which has been shared by other authors. It may be that there are certain magazines whose editors are chronically guilty, but I am more inclined to believe that it occurs with newer, unmellowed editorial personnel. Several years ago, I also had a paper marked up with hundreds of corrections by the editors of a technical journal of whose Publications Committee I am a member. Almost at the same time, another of my papers prepared with only the same amount of care was accepted and published in the American Journal of Science with no more than half a dozen changes. If the editor in the current controversy believes that his detailed editing is necessary to protect "a literary tradition, as well as standards of scientific excellence," he might contemplate the fact that the American Journal of Science is now 150 years old.

R. E. BIRCH
Harbison-Walker Refractories Company
Pittsburgh, Pennsylvania

The dispute between Dr. Key and the editors of the Quarterly Review of Biology points up what seems to me the worst feature of certain editors of biological journals. I do not have personal experience with these particular editors, but I have had experience with numerous referees and editors, as well as experience as a member of editorial boards and as actual editor of two journals.

The aim of using a referee system is to improve publishable papers primarily by providing the author with the advantage of outside criticism. This criticism presumably operates on the scientific level only, not the literary level—unless one classes ambiguous statements as "literary" rather than "scientific." Most referees confine themselves to the science. A goodly percentage of editors confine themselves to the content of scientific import, to serving as intermediary between referee and author, and to the necessary dealings with the printer. Sometimes there are additional legitimate editorial questions.

Most editors of biological journals are amateurs who perform the task either from a sense of duty or for the prestige resulting. Several of these have impressed me as being very good (an amateur can, of course, be an expert). A few lax editors do little more than act as intermediary between author and printer. But a few of the conscientious editors feel that they are called upon to "polish up" the manuscripts. This usually means rewriting the way the editor would have written had he been the author. When I have disagreed and been sufficiently annoyed to have such changes checked, the outside verdict has been that the change was unnecessary or that the change was inferior because it introduced a discordant style. Sometimes the change actually introduces error, that is the editor changes the wording to make the author say what the editor thinks the author ought to say! Such habits are not merely undesirable, they ought to be intolerable.

In publishing a book recently I found that the University of Minnesota Press had many editorial rules and was more concerned about adherence to textbook-type grammar than I. They queried numerous sentences and even insisted that a few be rewritten, but *they did not presume to rewrite them for me.* Herein lies good editing. A scientific manuscript, like a literary manuscript, is the author's baby; an editor is privileged to accept it or not, to insist on certain rules being followed, and to make suggestions, but he is not privileged to make it into the editor's baby.

On the other hand, many biologists, (at least in the U. S. A.) have not been effectively trained in the art of writing. Commonly an editor has the problem of what to do with a manuscript that seems to represent a good piece of research but is poorly prepared for publication. Even when a manuscript is acceptable, improvement is usually possible and most authors appreciate improvement, as the editors of Q. R. B. point out. I suspect this is usually true both when the changes are "correct corrections" and when they are just recognizable improvements. Certainly I feel that my own papers have been made better by both true corrections and improvements in wording suggested by editors, reviewers and other critics. But these desirable changes have been outnumbered by unnecessary changes to another's mode of expression and by "incorrect corrections." Editors have real headaches and are usually pressed for time but even so they should remember that they are editors, not ghost writers.

A. GLENN RICHARDS
Department of Entomology and Economic Zoology
University of Minnesota
St. Paul

Scientific Book Register

Progress in the Chemistry of Organic Natural Products, Vol. IX. L. Zechmeister, Ed. Vienna: Springer-Verlag, 1952. 535 pp. Illus. $18.80; clothbound $19.60.

The Mathematical Theory of Relativity. Reissue. A. S. Eddington. New York: Cambridge Univ. Press, 1952. 270 pp. $6.00.

Traité Elémentaire de Physiologie Humaine. 3rd ed. Henri Fredericq. Liége: Vaillant-Carmanne; Paris: Masson, 1952. 812 pp. Illus.

The Sensory Order: An Inquiry into the Foundations of Theoretical Psychology. F. A. Hayek. Chicago: Univ. Chicago Press; London: Routledge & Kegan Paul, 1952. 209 pp. $5.00.

Saudi Arabia: With an Account of the Development of Its Natural Resources. 2nd ed. K. S. Twitchell with collab. of Edward J. Jurji. Princeton, N. J.: Princeton Univ. Press, 1953. 231 pp. + plates. $5.00.

1953 Medical Progress: A Review of Medical Advances During 1952. Morris Fishbein, Ed. New York–Toronto: Blakiston, 1953. 301 pp. $5.00.

Physiology of Seeds: An Introduction to the Experimental Study of Seed and Germination Problems. William Crocker and Lela V. Barton. Waltham, Mass.: Chronica Botanica; New York: Stechert-Hafner, 1953. 267 pp. Illus. $6.50.

Food-Borne Infections and Intoxications. 2nd ed. Fred W. Tanner and Louise P. Tanner. Champaign, Ill.: Garrard Press, 1953. 769 pp. $12.00.

Textbook of Virology: For Students and Practitioners of Medicine. 2nd ed. A. J. Rhodes and C. E. van Rooyen. Baltimore: Williams & Wilkins, 1953. 561 pp. Illus. $8.00.

L. Farkas Memorial Volume. Research Council of Israel, Special Pub. No. 1, 1952. Adalbert Farkas and Eugene P. Wigner, Eds. (Distributed by Interscience, New York, and North Holland Pub., Amsterdam.) 309 pp. Illus. $6.00.

Mechanics of Materials. Seibert Fairman and Chester S. Cutshall. New York: Wiley; London: Chapman & Hall, 1953. 420 pp. Illus. $5.75.